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HOW DID THINGS BECOME COMPLICATED?
- Some methodological and ethical issues on the research of gossip

Abstract: This is an attempt for auto-evaluation of the methods that were applied during a research on gossip, as well as the ethical dilemmas that surround them. The hypothesis is that probably there will never be clear professional standards that should regulate, define and educate us in the "right" or "good" way of doing ethnological and anthropological research, especially when it comes to analyzing certain segments of every-day life, such as gossip. This situation, besides implying and offering a kind of freedom, at the same time could complicate our scientific work, and make it problematic.

Key words: field research, gossip, control, researches, interlocutors, ethics.

"Anthropological method is campocentric and autobiographical. We find ourselves in chosen fields, and we splice our previous experience into the research about others. How can it be otherwise? Why the embarrassment that some have expressed? If we felt so un-comfortable making representations of other peoples' lives, we should have chosen other work to do" (Шварц 2010: 23).

The research on gossip is a complex issue in the light of "generally accepted standards" (especially the ethical ones) in ethnological and anthropological research. The term "generally accepted" is in diverted commas, since such a thing in praxis is almost inexistent, due to the problems of methodological, technical, ethical and other types of regulation of field research. What could be defined as generally accepted, without being put in inverted commas, is the agreement that field research is an experience one cannot really foresee, that is, something that doesn't have to abide by the rules. Thus, one could think that there is no need of discussing the uncertainty that the field research, and the work of ethnologists and anthropologists in general brings, when this is so clear and free of any doubt. However, I feel a need to share a part of my experience and thus illustrate this essential ambiguity of our work that follows us everywhere, since at each moment things are prone to numerous different interpretations.

Field research on the gossip as a mechanism of social control in folk culture of Strushki Drimkol took place periodically during 2006 and 2007. Previous participation in two other research projects in the same region has made my “entry” there easier. Besides, the origin of my close relatives from this region has additionally influenced my proximity and my status of a partial “insider”. The research was done in company of my close relative, who originates from the researched region, and had a function of an interlocutor, guide through the village and provider of contacts, mostly by her own choice, through her relatives, friends, acquaintances, neighbors etc. (Ашталковска 2009: 10). In this way the research groups were created spontaneously. When you arrive at the village, you simply have a need to spend a certain time with your own “folks”, and you have coffee with them at their house, or they come to yours. When this type of atmosphere is additionally freshened with the presence of someone that presents herself as a researcher, interested exactly in such events, then the ambition to tell “unbelievable” stories is seriously motivated.

In defense of the absence of traditional conditions for research (that the place of research and people with whom we contact during our research should be sufficiently “distant and strange” in order to achieve higher “objectivity” of the research), which are promoted through ethnological and anthropological tradition, I will use the arguments that are in favor of the “new view”, i.e. “the new scientific understanding”: reflectivity or auto-reflectivity, which the researcher should continuously train through his/her research, and the relativity of the phenomenon of cultural differences. In this sense Gert Dressel has an interesting and picturesque note: “cultural differences exist not among different ethnic groups, but also in the frames of ones own cultural context – among the socially high or low cultural strata, among man and women etc. Today a 80 year old female villager from the mountains, that had spent a major part of her life in Tirol, in the village yard, for me – born in the city, socialized in the student-intelectual strata of the big town – is almost strange” (Дресел 1997: 85; (see also Ашталковска 2009: 10). This is one of the obvious differences that complicated the definition of my “insider” status: I conduct research in my own country, in the region where members of my family originate from, but I cannot identify myself as a complete outsider in many circumstances. Different statuses are formed and reformed in the process of negotiation during a field research, and accent sometimes the proximity and at other times the distance between the researcher and his interlocutors, based upon different criteria, depending on which status is in play at the moment: partial insider or partial outsider. Different statuses make a serious difference: regional background, religious affiliation, language, dialect, place of living (village, town, center, periphery), educational status, age, profession, employed or unemployed, man or woman… (the list could be almost endless). On the other hand, as prof. Svetieva notes, even when taking into account those serious differences caused by different statuses, it often happens that we succeed in establishing a relation with an interlocutor that otherwise “fulfils” all criteria of inaccessibility, in a strange and unexpected way, depending on the similarities or compatibility of the sensibility and the mental capacities of the interlocutors (in this case of both the researcher and the interlocutor. It is extremely difficult to identify the "pure" insider or outsider status, since in any circumstance things, places, people and the context are not purely unknown, but are also not purely known. In fact, things can never be fully known – a fact that justifies our "domestic" research adventures, that would otherwise not make sense (for an interesting illustration of this situation see Ergun, Erdemir 2010). Some say that places known to the researcher should not be investigated, since this (known) atmosphere could prevent him/her of seeing something new, and thus this situation is even identified as dangerous (Esterberg 2002: 64). In the concrete case such non-standard context of research, in the company of a closer relative, proved to be extremely useful for an easier and more authentic sharing of any type of information by the interlocutors, since in this way the construction of pre-established statuses of a researcher and informants is simply and promptly avoided. In the standard context of research however, the change of behavior happens quite often, in the sense of losing authenticity, and thus this behavior does not, any more, present its regular flow (Bialy Zarnecka, Bialy 1961: 13), but turns into a type of "emergency situation" (Ашталковска 2009: 10). In this sense, the entrance was much easier, acquiring trust went smoothly as well, and most of all, going behind the scene happened fast and went relatively deep. The "problem" in this situation was that most of my interlocutors, friends and relatives (that is, the elderly among them) found it difficult to perceive me as a researcher, but thought of me more as a "child" (although the child was 27 by then!), interested in different trivia of their life and their environment. This division of statuses could blur their awareness on what is really happening - this situation was on the other hand extremely useful for my research and the information I got, but at the same problematic when it comes to the way I used them. Surely, the way the interlocutors perceive the researcher (and the other way round) exercises special influence upon the research itself.

Techniques that were mostly used during this research were the ones of participatory observation and a non-structured interview. As Hammersley and Atkinson note, this "technique" lessens the influence of the researcher, and thus the interlocutors tend to open up on their perspectives and world-view (Hammersley, Atkinson 1983: 110). In this sense I will use the experiences related to group interviews, since they presented the base, or the best source for my needs. The groups consisted of minimum 2 and maximum 7 women. Our meetings lasted approximately 3 hours, but sometimes turned into whole-day sessions. The smaller groups were more productive when it came to gossip related to past events, but also when it came to individual gossip of the participants. In this way first-hand or direct data concerning the very process of gossip were available, as well as everything else that is covered by this type of communication. Interlocutors defined the flow and the topics of the conversation, because the main idea of the research was to “catch” the communication in its regular flow, starting from greeting, conversation between people which are in different relations, relations that involve different characters and intensities, the type of exchange information, when the gossip starts, how does this passage in communication look like, and finally, the communication session of gossiping itself (Аншталковска 2009: 10). A number of “guides” on qualitative research promote the following attitude: avoid positioning the interlocutors into pre-established statuses of a researcher and informants, where one should ask and the others should answer, since it enables easier and freer conversation, in which interlocutors are not put in a certain special situation outside their usual, every-day interactions. There was no need of a special method of recruitment of group participants, but the groups formed themselves spontaneously, they involved interlocutors that already knew each other and had a mutual history, easily remembered past events, reminded each other of some common experience, and expressed the semi-forgotten memories more openly, thus reconstructing them (compare Wilkinson 2004: 276). Thus, the research got a kind of “natural” and authentic dimension, having in mind that this looks like the usual, every-day communication, and because the interlocutors were not separated from the social context and the interaction with others. The exchange of experiences, especially among women, is a usual praxis in every-day life, and is thus considered an obligatory “relief” during the coffee-sessions to speak about problems, opinions, to express empathy, to open up, to self-evaluate, to justify, to confront – to deny what the other is saying, to remind her of her previous statements that are contradicting the current ones, to express disagreement, misunderstanding, and even spur argument, to question, provoke, change one’s mind etc. Due to these features, this type of work is considered extremely informative (Madriz 2003: 375). These every-day communication sessions often offer ground for exchange of highly intimate information, or exchange of data that are considered taboo by the wider environment.

In relation to the control over the research process, the terms such as “facilitator”, “moderator” etc. did not fit my role in the group interviews. Usually in literature one reads about focus groups, and the researcher has the honor to play the role of “facilitator”, i.e. some kind of group leader: “Focus groups are not ‘natural’ in themselves (they have not been spontaneously formed). They are facilitated by the researcher for research goals” (Wilkinson 2004: 277). In my case the research goals imposed minimal control by the researcher, since during the gossip sessions I usually ad the role of silent participant, whose role was to carefully listen, and pose, from time to time, certain questions, since communication depended on the interests of the female participants in the group, that on the other hand had their own important discussion topics. Due to the need for clarification of someone's identification I sometimes had to pose questions that leaded the conversation in a different direction, but the same happened even when it was not provoked by me. In this way the power is distributed among the main participants, i.e. among the group members, depending on their abilities for managing the relations during the gossip sessions. The researcher is not imposing himself/herself as someone who is higher on this power ladder (especially if the researcher is treated as an interested "kid"), but the structure of power is created by the group itself (Madriz 2003: 363–387). As Shantek puts it, the "creation" of the field is not an individual strategy of the researcher, but an important, and sometimes a major role in this construction of the field is played by the interlocutors whose decisions are crucial for the research flow. They decide whether they will allow entrance of the researchers into their environment and into their lives, they decide on what they will share and what will remain unsaid, they establish the limits, decide when communication starts and when it finishes etc. Thus, control over the research process is heavily dependent on the will of the participants, mainly because the ownership of the data, their quality and quantity, is in their hands, and because the exchange will depend on the relation between the researcher and the participants (Šantek 2006: 57; Karnieli-Miller et al. 2009: 282). The most important condition to gain entrance or insight into the gossip among members of a certain community is competency. Having in mind the fact that there is a certain specific language characteristic for each community, when it comes to gossip as a form of communication, certain pre-conditions are needed to successfully follow this activity. This means that certain criteria are needed to attain this competency, as for example: knowing the social context, which presupposes ability to recognize and locate the activities of the individuals that are subject of gossip, according to the community's hierarchy of values, i.e. knowing the cultural or social norms of the community (Ашталковска 2009: 11). Spending time with the interlocutors and learning from them certainly helps. On the other hand, the partial insider status meant that I already know the basics, and in this sense my questions sometimes irritated my interlocutors, since they emphasized the contrary – the unexpected ignorance on the basics. When they gossip, people use a strategy to identify the subjects to who gossip refers. Thus, one has to be aware of the principles for identification that the researched community uses. Since those who gossip are members of the same community, and are relatively informed on their co-villagers, at last concerning their identity, when a topic is opened it is quite normal that they don't need to explain things as they would when they would talk to strangers. In this sense, at the beginning of the research, until family, friendship and all other relations that were relevant for the conversation, I've experienced different difficulties in following the villagers' communication (in relation to my competency to understand the gossip). For example, clarifications concerning the identity of each person mentioned in the conversation were needed to understand the relations, the context and everything else surrounding the story of the mentioned person. In this sense higher concentration was needed, especially at the beginning of the field research, until I got into identifying of different persons: “Her husband and me, we are second cousins” is the simplest form of explanation of someone’s relation. However, explanations often do not finish here, but one goes in even more details, and further in the process of identification: “Her husband’s grandmother, the mother of his mother, is grandfather Krstan’s sister. Grandmother Martina” (Ашталковска 2009: 11).

With some of the interlocutors I succeeded to establish such relations that proved to be exceptionally good, and was identified as such from both sides since the beginning of the research. These people have thus become key interlocutors, not only due to the quality of information that I got from them, but also due to the quality of our relation. It is logical that in such conditions one would prefer to be around them, and thus, having in mind the intensity of our contacts, the relations turned into “serious”. This means that we already have certain common history, since we have mutual memories, we can laugh at some mutual experiences, deeds, situations, that makes our field work at the same time a beautiful personal experience, but also a potential problematic professional experience.

“Are not privacy and intimacy the most cherished experiences of identity?”
(Шварц 2010: 171)

In situations when your status of insider is “on”, when you attain partial competency, when you listen and when you are mainly silent, due to which you are experienced as a “child” that is interested in their stories, when you are not “dangerous”, and when communication can become more private and more intimate, at this moment it becomes at the same time more dangerous in an ethnical sense. Surely, almost every researcher has faced such ethical dilemmas, since „...collecting ethnographic data never takes place in an orderly context in which all ethical issues can be foreseen, and guaranteed through specific protocols“ (Nilan 2002: 381).

By now I emphasized the control over the research process by the interlocutors, but it is important to emphasize that as in anything else related to our work, things are not black and white. Thus, arguments can be found on the contrary situation – when the researcher uses different strategies and takes the power from the hands of his/her interlocutors: puts different “masks” on his/her face, can play the role of a nice person that wants to spend time with potential interlocutors, to participate in their activities, to fake proximity and friendship etc., and in this way he/she stills their trust, which surely could be treated as a demonstration of power. The same features of power ownership that were previously ascribed to the interlocutors can be taken back in the hands of the researcher. Although this is a vulgarization, the point is that there are situations during field research when the researcher has the last word, and plays the main role (Karnieli-Miller et al. 2009: 282, 283).

In sense of transparency, besides the strict regulation imposed by different ethical codes respected in academic environments with bigger tradition of ethnological and anthropological research, besides the heavily discussed (and at the same time quite controversial) problem of informed consent, still a silent legitimacy is given to these procedures that are otherwise considered "illegal". "...We can never be transparent, not only to others, but even to ourselves" (Smith, Deemer 2003: 446). We can never be sure how will our interlocutors understand our explanations at the beginning of the research, or if they will understand the implications that it could bring, since we are not mind-readers (although sometimes it seems as we are). The situation in which even the researcher "cannot understand oneself" in this phase of the research is legitimate, simply due to the nature of ethnological research. However, besides all explanations and justifications that we can find in relation to transparency as a phenomenon, it is true that we often use different strategies to "enter" among interlocutors. This problem lies among those to whom a silent legitimacy is given in ethnological and anthropological community, and is being heavily discussed in this frames: at the beginning of the research, and during the process of offering information, researchers use explanations that refer to the less painful, more naïve aspects of the research. For example, they say that they collect stories instead of saying that they deal with topics such as social control, they collect words instead of gossip etc. In this way the information that are given by the researcher are well dosed, i.e. one calculates the effect that these explanations might have upon the "entrance" of the researcher, and upon the field research itself (Thorne 2004: 162; Хамерсли, Аткинсон 2009: 57). At the first few meetings with potential interlocutors I tried to explain that I want to understand how communication flows, how news are being transmitted, what is the function of gossip in this context. After I got strange looks from my potential interlocutors, I changed the strategy, again with the assistance of some of my interlocutors. For example, some of them summarized my research goals as follows: they understood that I needed words that will be derived from our conversations, without identifying the lips that have uttered them. This explanation has fully satisfied me, since it was logical in relation to my topic of research, and at the same time it was logical for my interlocutors. But even when we try to be as transparent as possible, there are a number of other segments of our work that cannot be understood by our interlocutors. Generally, they often do not understand what our work implies (what do we do with the recorded material, the field notes, what can be done with the stories that we are told etc.) In relation to the latter, my interlocutors knew that I need communicating with them in order to write a book, that will be evaluated by my professors. This was an even bigger motif for them to help me get the best possible marks.

The promise of confidentiality and anonymity is surely the best argument used by the researcher, especially when it comes to sensitive research topics. Anonymity provides feeling of protection and security, but at the same time emphasizes the attention, care and the special treatment of interlocutors, and among other things this makes it most effective. Still, there are risks concerning identification. For example, a document on informed consent could include the following: "Although disclosure of your identity is a possible risk, every precaution will be taken to protect your privacy and the confidentiality of any records generated by this research. (...) Your name and any other identifying information will not appear in any reports of documents that are published as a result if this research project" (Esterberg 2002: 235). Different researchers use different measures to cover the real identity – they use fake names for the place of research, some burn or in other way destroy all materials after they have analyzed them (field notes, audio materials, transcriptions etc.). Confidentiality and anonymity are considered a basic norm in qualitative research in general, even in situations when one does not have an obvious need for that, i.e. even when informants do not have such need (see the article of Swartz in the same issue of this journal). The questions of anonymity and trust could be additionally complicated when, for example, it comes to group interviews, that is, focus groups. If in these circumstances one discusses touchy subjects, then additional measures of precaution are taken by some researchers: participants should sign a form on their obligation to respect confidentiality and not disclose information on the conversation or the interview with the focus group (Esterberg 2002: 111). In the context of gossip research, such strategy could mean that the interlocutors, after our first gossip session, should shut up, that the research should be canceled and another topic should be chosen!

After the communication between the researcher and the interlocutor is established, especially if it is of a private or intimate nature, things could become even more complicated. To get involve into such conversation, one should feel relaxed. All strategies that were already mentioned could contribute in obtaining and developing trust, developing the relation, so that interlocutors could talk to us as friends. "…In the comfortable atmosphere of the home and when there is trust, information that a participant might not have otherwise chosen to reveal might be so. (…) The stance of "interviewer as a friend" rather than an impersonal professional crosses conversational trust boundaries and may entice the participant into providing information that they might later regret" (Larossa et al. 1981, according: Corbin, Morse 2003: 338). Such relations could also impose certain difficulties. Thus, for example, the friendly relation could make the subjects forget that they are subjects of research, and treat the researcher only as a friend. Researchers usually do not try to react to this forgetting of the goal, since they are also pleased by the trust and acceptance, and what they are probably most pleased by is the easier and more open exchange information with them. However, the fact that the researcher is something more than a friend complicates things. Not only that the interlocutors, but also the researchers are victims of the same amnesia. Thus, interlocutors share something as if they are sharing it with a friend, and then he/she uses is when he/she is in a role of a researcher. Generally, there is an agreement that this is not exactly ethical, but at the same time many of the researchers practice it when "faking friendship" is a part of the strategy of free conversation and obtaining "juicy" field material (Thorne 2004: 167). Finally, this is unpleasant for us, as researchers. In the context of the exploiting function of the research, although it is generally admitted that there is some reciprocity in gain and loss between the researcher and his/her interlocutors (i.e. that both sides surely gain something from this relation), still the researcher is the one that gains the most. These complications are simply endless. "...When participants experience hardship or personal troubles, researches are in an ambiguous role. Should they act as a friend? Should they observe and take notes? What does it mean when a 'friend' takes notes about another's hardship and than benefits from it? When bad thing happen to research participants, it may simply be, to researchers 'grist for the ethnographic mill' as Stacey puts it" (Stacey 1996: 92 see: Esterberg 2002: 51).

"Is it justified to poke into the back stage because failure to do so will result in a superficial study?"
(Barret 1997: 228)

During the phase of analysis and interpretation of field data, control usually lies completely in the hands of the researcher. „From now on, the story shared with the interviewer is "separated" from the participant, and the researcher becomes the "storyteller" who recasts the story into a "new" historical, political, and cultural context". Now ethical dilemmas reach their maximum, when it comes to dealing with intimate topics. The inclusion of the participants in the research will depend upon the researcher (Karnieli-Miller et al. 2009: 283). There are equally valid arguments pro and contra restitution, i.e. pro and against the practice when the interlocutors have the honor to "check" the interpretation of the researcher before it becomes public. Some argument say that through the procedure of restitution one could obtain additional knowledge, check the validity of interpretation, clear the problematic parts of the study, identify the elements that could potentially harm the interlocutors etc. It is interesting that when it comes to restitution the following type of issues arise: which parts of the analysis could be presented to the participants without any hesitation? On the other hand, the justification states that the interpretation of the researches surely did not reach wider public, but only limited academic public, and thus the probability that the subjects of the research could get to it is small, since their influence upon social researches is really minimal. A third strategy enables interpretation which is free of worries concerning the reaction of the researched ones – when one writes in a foreign language or for a foreign, and again restricted, public (Karnieli-Miller et al. 2009: 283; Хамерсли, Аткинсон 2009: 212; Wiles et al. 2006: 296; Čapo Žmegač 2006: 229). An important note has been made by Chapo Zmegach, who refers to the caution in publishing of the interpretations, when the researcher lives in the same society as the researched ones, since in such conditions he is more vulnerable when the intimate life of another person should be interpreted. In such circumstances his responsibility is emphasized, since dealing with eventual consequences is more probable (Čapo Žmegač 2006: 229). Still, can ethical responsibilities adjust to the above mentioned cases? Is this adjustment an example of ethical behavior?

I am not sure that the restitution would function in the concrete circumstance of gossip research. I did not try to do that, but there is an indicator that allows me to suppose if the restitution would function or not. For example, my relative, who was directly involved in most of the research, did not have any intention to read the book, although was very proud to show it as a product in a form of book on which the name of her niece was written. Restitution would hardly work if interlocutors are village women around 70 years of age, some of the illiterate, meaning that they do not have the habit of reading anything, not to mention a book, even if the book mentions them. Is this a sing of absolute trust or a total lack of interest in what certain researches write about their culture? If the first option is the real one, then it means that the trust of the interlocutors is often misused, and if it comes to the second option, then their lack of interest is maybe an advantage for them and for the researcher, in the sense of avoiding engaged discussion concerning the representation of their words and their lives, avoiding accusations and justifications. I am sure that many people would not be satisfied with my interpretation of the field materials, and this goes not only for the direct participants, but also the ones that would eventually recognize themselves as participants in the gossip sessions. The misunderstandings between the researched ones and the researchers when it comes to interpretation are surely common. "Restitution unavoidably leads to misunderstanding exactly because of the fact that the type of objectification that we do is either incomprehensible or unacceptable for the researched ones, who could reject it in the name of their own truth" (Čapo Žmegač 2006: 231). For example, Borland interpreted a narrative totally differently than her grandmother: "How, then, did we, who had a close, confidential, long-standing relationship, manage to misunderstand each other so completely?" (Borland 2004: 531). It is true that no one can foresee with mathematical precision what could harm the interlocutors, and thus the "harm" done by the researchers is unwilling.

The privilege and the satisfaction that stem from the well established relation with our interlocutors, through which they share private or intimate stories, goes hand in hand with heavy responsibility, towards the interlocutors themselves but also towards their stories (Dickson-Swift et al. 2007: 340). This heavy responsibility asks for caution in representation of other people's lives, since in a contrary case this could "…may constitute an attack on our collaborators' carefully constructed sense of self" (Borland 2004: 529). An eventual dissatisfaction of the interlocutors that would eventually read the researchers' interpretation (in case of lack of restitution) does not have to stem from the "precision" or the "invalidity" of the content of the representation, but from "...what upsets people is that they have been put under the microscope, treated like scientific specimens, and in that way dehumanized" (Barret 1997: 192). Our work often presupposes a situation in which small private moments of the interlocutors are finally put in a wider context, i.e. stories are made up based upon their intimate statements in a form of articles, books etc., that present a public work. Thus, "intimacy has to do at the same time, with the most private and the most publicly regulated acts and practices" (Moreno Figueroa 2008: 76). The emphasized precaution, concentration and care regarding the reactions of the interlocutors, intended not to harm those that have shared the most intimate details with us, is easily declared, but it is difficult to implement in praxis. The strongly desired and promoted personal interaction with the interlocutors at the field, that is required by methodology, becomes "tricky" when we exit from the field, since it creates different inter-personal difficulties (Guba, Lincoln 2004: 33). In different phases of the research process (field research and its interpretation) intimacy is located at the desired, i.e. not desired extremes.

The misuse of a certain idea of intimacy crosses the red line of good behavior. I have done this, few times. Such confessions (I admit that they are partly initiated by bad conscience, as well as the motto "the one who admits is half forgiven"), should still have a didactic dimension, i.e. should serve to draw conclusions from someone else's experience, should be useful for the ones who are read to learn from other people's mistakes, but also for the "sinners" themselves.

Gossip is closely related to the concept of intimacy. According to its "effects" and functions it simultaneously supposes or supports intimacy, but also what is contrary to intimacy – distance. In the first sense gossip enables intimacy in the following ways: through revealing data on the gossiped ones, i.e. on other people, that often penetrate deep in their private sphere; and though establishing intimacy (or redefining and confirming intimacy in that way) with the interlocutors through gossip. "Talking about someone who is not there is a way of establishing rapport with someone who is there" (Tannen 1990: 107, according to Funda 1999: 96). The researcher is already included in the gossip sessions and is someone who can be trusted, since in a contrary case the interlocutors would be much more careful. If intimacy is closely related to the revelation of oneself in front of another person with whom we are in an intimate relationship, then intimacy often supposes a revelation of a secret. However, gossip does not only include a revelation of a personal secret, or a secret of the teller, but a much easier transmission – of someone else's secret. "If someone was to share something personal and private with another, what is the likelihood that secret would be passed on to a third party? Although few care to admit it, many are guilty of transmitting such secrets" (Yovetich, Drigotas 1999: 1135). In Strushki Drimkol the term "retelling" (prekazhuvanje) illustrates this type of communication. It includes first hand transfer of information, i.e. the one who transmits it is directly responsible for its dissemination. The conversation presupposes certain secrecy or confidentiality, even when this is not directly required by the teller. Re-telling means breaching the trust of the speaker, and transmitting his/her words to the community, in worst case transmitting the information to the ones to whom they referred, while gossip presupposes a wider context, that does not have to include the direct participants. Information that is considered secret includes precaution during transmission, but this does not mean that by the end they would really keep the status of secret information. Information that is characterized with certain peculiarity simply asks to be "freely trafficked", that is, almost no one can resist the opportunity to share them. Thus, it often happens that the ones who are well acquainted with our "secrets" are in fact the ones least expected. This practice, as this text shows, is not only reserved for the traditional village community (Ашталковска 2009: 95, 96). Surely, certain preconditions for transmitting another persons' secret is needed (for example, a close intimate relation of the ones that transmits the secret to a third party) since, in the contrary case, if these norms are not respected during the sharing of a secret this could lead to serious consequences for the person that transmits, even to ostracism from the social circle (Yovetich, Drigotas 1999: 1135, 1136). This circle of relations is a warning, that one should closely monitor ones ethics during the research of intimate topics, but also the ethics of our every-day intimate relations. Thus, this topic due to its intimate nature immediately suggests that the use field material should be done with great caution.

The main point of the whole presented ethnographic material was not to reveal the secrets of my interlocutors, but to support the thesis of Havilland that gossip is a dictionary of ethnographic facts related to the researched community. In this way one elegantly attains entrance and insight into literally all spheres of life, simultaneously offering the breaching of the conventions for proper behavior, but also the conventions themselves. It is logical that, when the research focuses upon what is behind the scene, and thus presupposes some negatively perceived acts, there is a great danger of dissatisfaction, initiated by what came to the open and became available for the wider public. Discussions related to the consensus between the researcher and the researched subjects in this sense are still valid. As Barret says, the results of the research always have at least two types of public: the academic one, and the one that has previously been a subject of research. But the question for whom one should write is difficult to answer. “To please one of the audiences may not be difficult. To please both may be almost impossible” (Barret 1997: 203). The solution of the ethical dilemmas consisted in respecting the principle of anonymity, i.e. using of pseudonyms for the interlocutors, for the persons mentioned in their gossip sessions, as well as pseudonyms for the mentioned villages. In the phase of interpretation another new problem emerged, having in mind that anonymity is not sufficient to prevent someone’s identification. Sometimes a small amount of details allows the person and the place to be identified. “The researched ones would probably recognize themselves in the quotes, as well as recognize their neighbors” (Čapo Žmegač 2006: 235, footnote 20). Although I thought that the “examples from the field material are carefully chosen, so that identification of the participants or the place would be avoided (as much the research on gossip allows this)” (Ашталковска 2009: 13), still, as suggested by the brackets, I haven’t respected fully the principle of careful choosing of the examples. Auto-censorship means that not everything should be published, even when it is extremely interesting, if this implies a danger of breaching the privacy of the researched subjects. However, being careful in this sense could at the same time present a manifestation of the controlling power of the researcher. For example, my interlocutor is intentionally sharing data that are offensive for her daughter in law, since she wants everyone to become aware of their problem. It is almost sure that, if the daughter in law becomes aware of this, the conflicts between them would surely get momentum. Should this gossip session be censored, and what would the goal of the censorship be? For the good of the community? For the good of someone’s daughter in law? Should the caprice of my interlocutor be satisfied, who among other things spent long hours retelling me details of her life? Do the research or the story themselves play a role? These, and similar questions, are specific for almost all examples from the field materials, simply because it is a matter of gossip. I was deeply involved in the gossip sessions, no matter if I was only a tentative listener or in this special way: “it often happens that the researcher behaves and reacts as those villagers that are identified by the environment with the local term ‘curious’, i.e. as people that are interested in every detail. People with such habits are usually related to the practice of gossip. This is how the researcher’s interested looks like in its lightest form: A.A.: And this woman, who was she? With whom does she live? And why did she act so strangely, as if…Why is she a bad person, why? And is she in contact with her son, which got re-married? Where is he? Does she talk about him?; or: And who is Bojan? Why is he a fool? Why do you have an argument with Bojan? In this sense it is important to emphasize that the topic of interest should not be treated as something specific for this specific researched region, since gossip as a form of communication is probably valid for everyone, no matter the time and the space (…) Such similar reactions to the above mentioned example, when the researcher is identified as a “curious”, or above average interested person, is contained in a number of field researches. But the difference is that people that look for information about other people are often thought to misuse these data for their transmission or for gossip, while such interest of the researcher is probably a professional deformation. I hope that the research ethics in this sense will be on a high level, and that the information obtained will not be misused” (Ашталковска 2009: 13, 14). Besides my sincere hopes, the result of the research in a certain manner became a classical example of the gossip as a mechanism for social control, having in mind that I’ve put myself in a position to eventually control the relations between people that would read the book once it gets published, and that would identify themselves (and the others) while doing it. In this way the researcher is fully involved in the construction of the relations inside the researched community, having gossiped with other members of the community. However, could it be any different when gossip is the topic of the research? How can the positivistic realism be avoided, without telling fake, or made up stories, through which we would make the point, only due to the fact that fake stories look like the “true” ones, the ones that the interlocutors have really told us? Finally, does it mean that certain topics are not especially suitable for research, since they could harm someone? Sonja Potkonjak well notes, summarizing these tricky dilemmas: “Did the obligations to satisfy the stances of the ‘epistemological partners’ in the ethnographic research and the later textualization lead to a situation where the ethnologist does not posses any more the autonomous decision on what is allowed and proper; do the tracing for the still unconscious, hidden ‘truths’ escape the ethnologist, while the public interpretations which are suitable have become the only appropriate places for research and forms of textualization, or the reformatory self-disciplinary bon ton…” (see the comment of Sonja Potkonjak in: Zebec 2009 : 32) It is exactly the ethical dilemmas (that sometimes look unsolvable) that justify the unpleasant feeling that many of us have during the representation of other people’s stories (and other people’s lives!).

“Once the anthropologist gets accepted, the communication becomes more personal and more spontaneous, and in this moment the gathering of information works the best – when people become as important as the stories they are telling”
(Karim 1993: 82)

In an ethical sense this statement means that people should become as important to us as the things they are saying, but this can again become complicated depending on how we calculate importance, if we are going to do that at all. Who will be then considered important – only those people who give us important information, while the ones who provide us with less information will be considered less important (since this can also happen)? The point of this quote could be clarified through an important note, which says that the information of an intimate nature are not only transmitted or recorded on a voice recorder, but that this information are pointed towards the researcher as a human being, who has to react in return as a human being (Dickson-Swift et al. 2007: 335).

Ethics is a changeable and pretty unstable category, in spite the attempts to pack the ethical professional behavior in social sciences and humanistic in a form of ethical codex. What is considered an ethical behavior today can easily not be identified as such in the future. Even what is considered ethical today could be red and adjusted in different ways, since the written ethical rules often do not correspond with the ethically problematic field situations. As Moreno Figueroa says, the different interpretation and the different use of ethics often contradicts the research process (Moreno Figueroa 2008: 76). Especially when sensitive topics are analyzed, ethical dilemmas could confuse us, since our responsibility is not limited only to the participants, but it also concerns the data, the analysis, ourselves, the public, the academic community etc. According to contemporary ethical "standards", much of our humanistic research could be estimated as lacking humanity. Thus, humanistic disciplines become highly risky for humans – for the participants/subjects of the research, but for the researchers as well. "Having in mind that the objects of anthropological examination are in fact human subjects, and that there is a certain reason why they are considered as different from the objects that are covered by physics, chemistry or astronomy, anthropology is a highly risky and sensitive humanistic discipline" (Tepavčević, Milenković 2008: 200).

Ethics is a hot topic in the frames of Western academic scene. In this world ethical issues are strictly regulated in a form of ethical codex and institutional revising council, but they are also strictly interested when people are put in a situation to research other people. Such strict regulation of ethics in social sciences and humanistic could be also quite unproductive, primarily due to the very regulation of something that is hard (or impossible) to regulate, and secondly, since regulation itself can induce limitations in regard to the interests and topics of research. For example: "because of perceived (and sometimes actual) difficulties in receiving IRB approval, some researchers have shied away from controversial topics or innovative research designs" (Esterberg 2002: 50).

Besides the ethical regulation of research in a form of codex, the personal estimation of ethical behavior is highly valued, since it enables peaceful sleep when we go to bed at night: "…each of us daily, through each decision made, bear the burden of moral values of the community in which we live and research, and in this sense we are obliged to analyze and continuously estimate them, for our sake" (Zebec 2009: 21). "Today I can confirm that the ethnologist, above all, is responsible for the (changeable) standards of his own discipline and of himself!" (Čapo Žmegač 2006: 232). However, even this equality of the personal/individual and the professionally established consensus on the research ethics is problematic, since to be an ethnologist and an anthropologist does not necessarily mean to be good, just, ethical, fair… player (see the comment of Sonja Potkonjak in: Zebec 2009: 15-33). Still, I join numerous researchers that identify the need of discussions, especially on the individual, practical, concrete research ethical issues, since, in this way, they obtain certain applicable (or non-applicable) feature.

Certain research practices have to do, to a large extent, with the history of the discipline in Macedonia and its praxis, having in mind the fact that we mature through the directions that it gives us. Generally even today there is no great interest among Macedonian ethnologists and anthropologists to debate regarding methodological issues, so one cannot speak about a critic of past practices, which could otherwise be very useful. As Bakjevic says, the critics of past practices in a methology and theoretical sense could be extremely useful if one does not try to find the "guilty" part, but if the critic is applied in changing of the present (Баћевић 2007). Up till now I analyzed myself and my own research practice, but now by the end I would like to share the responsibility with the wider community of Macedonian ethnologists and anthropologists. I believe that many of my colleagues will identify with the presented problems, since we all belong to a small, young, and thus not very competitive community.

In the Western (mainly American) world of the discipline such auto-evaluations of one's own research methods and methodologies have started from the 1960ties and are valid even today. The revolt of the 60ties expressed on the western stage was led by young scientists who found this as the only way to express dissatisfaction by the lack of "directions" and "manuals" for performing a field research. Professors, instead of providing a sum of techniques, advised them to take with them a lot of paper and pens, to take care that their shoes are dry and to avoid sex with natives. The main point of the explosion was the demystification of the qualitative research, when all its aspects were put on paper (Barret 1997: 108). Such a deministification has not yet taken place in our local discipline. There is a serious lack of literature that treats field research, although this practice is always taken for granted in the articles of ethnologists and anthropologists in Macedonia. This can be a result of the fact that the field research is something taken for granted, but this is some kind of necessity, that enables writing, something that cannot be avoided or skipped, and it is perhaps the reason why it is considered that there is no need of "burdening" the text with something so well known, obvious and granted. It is a paradox, but field research is at the same time something very mysterious and something very familiar. Barret promptly started to lose the illusion on the importance of such literature, and for good reasons. The attitude of early anthropologists, namely that students are learning about methods when they conduct their own field research, is justified. "Before one gets one's feet wet in the field, the literature on methodology serves as a security blanket; after one reads it in a different light - it is transformed into data, theory and epistemology". Another important reason for losing the illusion is the big contradiction between methodological literature and the very field research: "Rather than being a linear activity, with each stage unfolding in logical sequence, fieldwork is a nonlinear activity. It is governed by unexpected shifts in focus, accident, good and bad luck, false starts, sudden insights, and personal, subjective experiences unique to the fieldworker" (Barret 1997: 109, 110). Surely, practical knowledge and not just theoretical is important. But these two aspects should not stand one after the other, but one next to the other.

The positivistic view of the world, according to which the researcher can be impartial, objective, neutral observer and interpreter, has been abandoned long ago. The terms such as proof, objectivity, checking of results, truth etc., are not functioning any more, but in praxis there are remains of their manifestations, as traces of the positivistic past. Objective scientific perspective presupposes that the ethnologist or anthropologist has been at the field, has done talking, and in fact has done more listening, registering his account of the culture first hand, so that his analysis represents what the "natives" cannot see (Borland 2004: 523). Thus, the researcher is often represented in practice as a detective whose goal is to discover what others (including the participants) have not seen or have not understood up to that moment (Karnieli-Miller et al. 2009: 281). The concept of proof simultaneously calms research passions, having in mind that the special conditions and circumstances of knowledge shared by interlocutors are not the ones which are interesting, and that the same goes for the ones from the side of the researcher (for more see Hastrup 2004: 455–472). This principle initiates as a result an exclusion from personal responsibility of the researcher, since his own values are not taken into account, and neither his attitudes, feelings, statuses, subjectivity, or his position. For example, in an ethical sense this would mean that the interlocutors should not be angry if we speak the truth, and nothing but the truth. Thus, the responsibility for the things that they share with us is only their problem, since they agreed consciously to participate, and to even be recorded as they speak. The logic is: why should I not use their word then? This exemption from responsibility of the researcher is visible in few segments. For example, when it comes to academic writing, the use of passive voice and the plural is specific: “it has been shown that..”, “it was discovered that..”, “our research”, instead of writing in first person singular, which would emphasize the fact that a concrete person has conducted the research (Esterberg 2002: 208). Another aspect in relation to writing could also be connected to the above mentioned strategy. To illustrate it, I will use my own example: I have a strong need to use long quotes by my interlocutors. In the case of the gossip related research, there are moments where whole gossip sessions are quoted, and my words serve only as a guide, that is, as a clarification of what the interlocutors say. The explanation was the following: having in mind the fact that our research is often treated as interpretation of interpretations of the informants, this form of description, which focuses upon the voice of interlocutors, is the most suitable. On the other hand, besides the content of the gossip sessions, that is surely more “precise” when it is originally shared, instead of being retold by the researcher, the specific language of the gossip sessions is also more attractive than some interpretation of the researcher. Thus, among other things, the folkloristic elements of the gossip sessions become visible, and they give special tone and color (Ашталковска 2009: 14). Other authors, on the other hand, do the same to enable the public that will read the article to give its own interpretation as a last participant in the creation of meaning (Čapo Žmegač 2006: 224). There is a third type of authors that consider this principle of adding illustrative quotes to the writing as an inadequate form of ethnographic analysis, or as a lazy replacement for a proper analysis, and emphasize that in this way the field material is presented as a direct proof of the retold events (Хамерсли, Аткинсон 2009: 170). This is even further complicated by ethical problems, since the detailed quoting presupposes more details and, as a consequence, a bigger threat regarding the principle of anonymity and interlocutors. When we learn to deal with ethical issues, then we certainly think more about the content and the essential segments, we think things over, we discuss and we adjust it etc., but when we lack procedural knowledge, then it can easily happen that ethical issues are not taken into consideration as a factor in our researchers. This leads to disruption of peace not only with the individual researcher, but it also shakes the whole research community. “Through the production of different types of ethical discourses, from publishing ethical codex to debates raised by them, one silently produces the background and indisputable character of ethics, that opens the question on the regulative ideal of the discipline – if we had truth and peace up to this moment, could we expect in the future that ethics will become the regulative ideal of the discipline?” (Tepavčević, Milenković 2008: 201).

The norms regarding the need to go into details and precision in our research community are also visible through the treatment of interlocutors in the course of the research, but also after it. For example, it was very important that they would be identified as sources of data in the sense of their name, surname, gender, age, place of birth, place of living, marital status, when and where they got married, religious affiliation, national affiliation, education (which in our circumstances of doing research in rural environments often presupposes the question: “Are you literate?”). This is a result of the treatment of the data obtained by interlocutors as facts, proofs that should be subjected to objective scientific examination, i.e. “certain investment of positivistic objectivity and verifiability of research result” (for similar principle in Croatian ethnography see Čapo Žmegač, Gulin Zrnić, Šantek 2006: 41). It is the best when all of this would be audio recorded, since it additionally provides the “verifiability” of the material. In this context, at least for me, it was not a matter of estimation of the problem, if interlocutors are lying or not, but to “register” them, as a proof that I am not lying or inventing things. Most of my colleagues would probably certify that the audio recordings are still considered extremely important. A similar question is posed by Vesna Petreska, on recording as a technique that provides more evidence, and at the same time, that recording is sometimes an obstacle of intimacy and honesty in field research: “When we talk, they sometimes simply say ‘shut the thing off, I will tell you’. How can one scientifically interpret in such a case? Does this mean that I should not take into consideration additional statements that have not been recorded, since they are a type of ‘private note’, or that I should not register the situations where the animosity between relatives is evident, and thus present an idyllic picture characteristic for the ethnological research in the past, that has in fact never existed?” (Петреска 2009: 147) However, it is important to emphasize that to reveal at least a part of the identity of the interlocutors is legitimate. The attention given to these problems lately (i.e. to anonymity and confidentiality by ethnologists and anthropologists in our country, is a indicator that the interlocutors slowly gain their honorary place in our research (for example: the MA thesis of Ines Crvenkovska Risteska, “Anthropology of male same-sex sexuality in the Republic of Macedonia”, IEA, Skopje, 2009; MA thesis of Elizabeta Pakovic, “Family album as a source for analyzing of the identity of the Miyaks”, IEA, Skopje, 2009; PhD of Davorin Trpeski, “Creation of cultural policy in the process of protection of cultural heritage in the Republic of Macedonia in the period 1991-2007”, IEA, Skopje, 2010 etc.)

According to the tradition of research in Macedonian ethnology and anthropology, the term ‘informant’ was and still is characteristic. This term is more and more disputed and discussed in the context of the terminology we use for naming interlocutors. This term gives a negative association, above all since it is associated with the ‘informants’ from the time of the ex-socialistic system – the ones who ‘leak’ information, the ‘denouncers’, that gave away information to the police and the authorities on the suspicious persons and enjoyed different privileges as a reward (for a splendid illustration on the misuse of intimacy through 'informants' by the institutions of our socialistic past see Котеска 2008). In our (research) circumstances informants are the people who are the source of information, upon which our whole work is based. When we say that information obtained from our informants are in fact the essence of our work, it is natural that both the informants and the information they provided are very important to us, which means that the term "informant", no matter how "official" it sounds, is not considered as such by the ones who work with those informants. This term is pretty legitimate, I find, if we take into account only this context of field research. However, during research of intimate topics, that presuppose a non-structured interview, that resembles every-day communication, the term informant does not fit this frame. In such conditions informants are not only those people who inform us, but they actively participate in the construction of the research process. The term *informants* then is associated with a more structured, strictly controlled atmosphere, in which their task is only to give information that would answer the posed questions by the researchers. According to postmodernist paradigms, which among other things deal with the relation between researcher and the researched ones, the accent is put upon the change of the status of the informants, who in the course of the research in which they started as objects gain the deserved status of subjects of the research – "…with their voice, views, and dilemmas, their own specific attitude and interpretation of situation and cultural meanings" (Čapo Žmegač 2002: 42 според: Čapo Žmegač, Gulin Zrnić, Šantek 2006: 33). A logical consequence would be a change of terminology, that does not fit any longer the status of the subjects, and that they turn from "informants" to "interlocutors". There is a different terminology in this sense in contemporary western anthropology and ethnology, that could also go into extremes, using for example terms such as "partners", "co-researchers", "co-authors", and even friends, surely depending on the world view of the researcher and his/her plan, estimating the level of control or the level of inclusion of the interlocutors in the whole process, and especially depending on their inclusion in the interpretation of the results. According to the differences in naming of the participants in the research, one could estimate the relation towards them by different social and humanistic disciplines that deal with qualitative research. "These differences highlight the variation in the many levels of the power distribution within the qualitative field research, suggesting that even in some qualitative research paradigms, participants are not always considered to be the 'real experts'" (Karnieli-Miller et al. 2009: 281).

Especially when neither in every-day life, nor in professional life, one can find at least a bit of "more secure option" for acting, when each move could be at the same time justified and blamed, then really - why so much discussion on this duality? A folk term from "my field work" is a perfect explanation of our situation - "nikakvata" (neither one), which means that any choice we make could still be the wrong one! Thus, feeling uncomfortable is an absolutely justified emotional state when it comes to a certain auto-evaluation of field experiences that include sensitive topics. Thus, I still don't want to change profession, but I can start thinking about dealing with something which is not so “sensitive”, although again things can get complicated, depending on the circumstances surrounding each topic, that could become “sensitive” later on.

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